LONG ISLAND BIOLOGICAL ASSOCIATION
COLD SPRING HARBOR, NEW YORK

THE BIOLOGICAL LABORATORY

October 21, 1955

Dr. Joshua Lederberg
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Dear Josh,

I am sending you a summary of the tests on Novick's chemostated 58-161. For some now unknown reason, I had been assuming that you had this information. Your suggestions as to the handling of the F-disinfection studies sound satisfactory. The first thing I had been intending to do was to try to disinfect by repeated passage in minimal medium with limited supplement, i.e. a fluctuating population never exceeding a certain population density. The use of a chemostat is much more sophisticated however and should answer any questions answerable by the other method. I have a number of things in mind with regard to this problem, but most of them are not directly pertinent to what has and is being done. One thing, however, is pertinent I think: In none of the motility-disinfection experiments did I have an extensive series of controls of the following kind. During passage on motag, perpetuate two lines of descent, one involving constant isolation from the periphery (as usual), one constantly from the center of the swarm. My guess is that both would eventually become highly motile, but that only among the former would F- be found. If this has not been done there, and you think it worth doing in light of what you have found by now, I'll be able to do this.

The B transducing system to be used involves P1. Involving as this does an initial emphasis on phagology (isolation of P1 mutants, etc.) progress has been agonizingly slow, despite the fact that Jill Hershey has been working with me on the problem. I suspect too that the S^d auxotrophy at least is due to mutations at other loci; but, if so, it will still be interesting to test the large series of independent mutants for "microallelism".

I suppose you have heard of the Salmonella "transformations". DNA from wild-type brings about the appearance of $tryp_1+$ among $tryp_1-$ (for example), but many of these "transformants" are now deficient at another tryp locus ($tryp_2$ for example). The activity of the preparations is destroyed by DNAase. Demerec suggests that the appearance of an allele present in neither donor nor receptor may be due to damage incurred in the preparation of the DNA (at the ends of small fragments for example). So far as I know, the critical control has not been done: Demonstration that DNA from, say, $tryp_1-$ does not also cause the appearance of $tryp_1+$ when applied back upon $tryp_1-$ cells.

Ozeki, in Demerec lab interprets a phenomenon in adenine transductions as analogous to "trailing" in motility transductions. Along with large transductants, he finds small colonies; upon restreaking, these never contain more than one colony-producing-cell on the original selective medium. Restreaking from this single colony again shows only one colony-producing cell present. If this is due to an active, non-multiplying fragment, I wonder if some of the small, non-prototrophic colonies obtained in coli crosses might be analogous.

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I haven't had a chance to do anything more on the Jacob type experiments, but hope to very shortly. I think the difference between the two experiments can be shown to be due to strain differences by the type of experiment we have already done: Using oru technique and using lysogenic Hayes' Hfr show that the order of transfer is L-V-Lac, as already suggested by our meagre results. I have sent you the culture of Hayes' Hfr Caren and I used; we obtained it from Lennox who calls it K15 (M^- Az^r S^r). Thank you for the cultures; I am a little confused at the moment as to what Alan may have had in mind. What would be most desirable, I think, would be a well-marked F- Lac- V^S_I T- L-; is such a strain available?

Your question with regard to La Jolla is appropriate; and I must admit that, after thinking it over, doubts accumulate. Much of this is due to my ignorance both of marine biology and the position itself. I have thought for a long time that I would like to attempt something on the developmental genetics of a colonial protist (e.g. Volvocales) and had thought that something of this kind might be appropriately done at La Jolla. However, the Volvocales are not marine and I really have no idea what organisms might be appropriate. Sponges perhaps but then these are not microbes. So this is the state of my ignorance. Granting that appropriate marine organisms exist, that their genetics is not too inaccesible, and that such work would be welcome, I think I could honestly predict a considerable interest.

Sincerely,

Dave

P.D.Skaar